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# Ideology, Inevitability, and the Scientific Revolution

*By John Henry\**

## ABSTRACT

Looking in particular at the Scientific Revolution, this essay argues that, for all their differences, positivist commentators on science and contextualist historians of science ought to be committed to the view that counterfactual changes in the history of science would have made no significant difference to its historical development. Assumptions about the history of science as an inexorable march toward the truth commit the positivist to the view that, even if things had been different, scientific knowledge would still have ended up where it is. Perhaps surprisingly, the move away from “great man” history and the increasing emphasis among contextualist historians on the broad cultural influences on scientific thought and practice also imply that changes of a restricted or specific nature ought to have no significant effect on general outcomes. Unlike the positivist, however, the contextualist is willing to concede that things might have been different if the entire cultural background had been different. But in such cases the effect of such sweeping changes would be impossible to conceive and so deprive counterfactual history of any useful insights it might be supposed to offer.

SUPPOSE THE CONCEPT of *actio in distans*—to which Newton was happy to subscribe, and which many eighteenth-century Newtonians accepted without qualms—had never been rejected in the nineteenth century.<sup>1</sup> For one thing, there would have been no need to develop concepts of non-Euclidean space to explain gravitational attractions. It was only in the nineteenth century that Newton’s famous comment, in a letter to Richard Bentley, that action at a distance “without the Mediation of any thing else . . . is to me so great an Absurdity, that I believe no Man who has in philosophical Matters a competent Faculty of thinking, can ever fall into it,” was seized upon and used to cover up the all too

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<sup>1</sup> Leaving the unpublished manuscripts aside, Newton explicitly invokes actions at a distance in the preface to the *Principia* and in Queries 1, 4, 18, 21, 29, and 31 in Book 3 of the *Opticks*. On eighteenth-century use of actions at a distance see, for many examples, Robert E. Schofield, *Mechanism and Materialism: British Natural Philosophy in an Age of Reason* (Princeton, N.J.: Princeton Univ. Press, 1970); and Arnold Thackray, *Atoms and Powers: An Essay on Newtonian Matter-Theory and the Development of Chemistry* (Cambridge, Mass.: Harvard Univ. Press, 1970).

obvious fact that, elsewhere in his writings, Newton was perfectly happy to assume—and even promote—the concept of actions at a distance.<sup>2</sup> Émile Meyerson pointed out long ago that the cover-up was based on a misreading of Newton's comment; but the nineteenth-century opposition to actions at a distance became so deeply entrenched that the misreading is now impossible to dislodge.<sup>3</sup> And yet, given the undeniable links between the use of actions at a distance promoted by Roger Joseph Boscovich and Joseph Priestley in the eighteenth century and the thought of Michael Faraday in the nineteenth, it is at least plausible that Faraday's concept of fields of force could easily have been envisaged by contemporaries not as a way of showing that actions at a distance do not happen but, rather, as a way of showing how they work. Although Faraday's "Speculation Touching Electric Conduction and the Nature of Matter" of 1844 was intended to reject actions at a distance, it could easily have been presented differently if prevailing attitudes had been different. After all, John Tyndall said that in it Faraday had "immaterialized matter into 'centres of force.'" In their evocation of a universe in which all things are connected, Faraday's own words have an almost magical ring to them:

The view now stated of the constitution of matter would seem to involve necessarily the conclusion that matter fills all space, or, at least, all space to which gravitation extends (including the Sun and its system); for gravitation is a property of matter dependent upon a certain force, and it is this force which constitutes the matter. In that view matter is not merely mutually penetrable, but each atom extends, so to say, throughout the whole of the solar system, yet always retaining its own centre of force. . . . Hence matter will be continuous throughout the universe.<sup>4</sup>

Counterfactuals like these, bringing out the contingency in science, could be almost endlessly multiplied. And yet, it may well be that whatever suggested change is put forward, it will always be possible to argue that overall things could still have, and would still have, turned out pretty much the same. In other words, there will always be those commentators on science who will want to insist that the history of science is the story of a gradual but inevitable progression toward the truth of how things are. Since there have been many wrong turnings, blind alleys, and unnecessarily circuitous routes taken along the way, there is no real reason to suppose that a diversion down a counterfactual blind

<sup>2</sup> Isaac Newton to Richard Bentley, 25 Feb. 1692/3, rpt. in *Isaac Newton's Papers and Letters on Natural Philosophy*, ed. I. B. Cohen (Cambridge, Mass.: Harvard Univ. Press, 1978), pp. 302–303. On the exploitation of this quotation by nineteenth-century physicists see F. H. van Lunteren, "Gravitation and Nineteenth-Century Physical Worldviews," in *Newton's Scientific and Philosophical Legacy*, ed. P. B. Scheurer and G. Debrock (Dordrecht: Kluwer, 1988), pp. 161–173.

<sup>3</sup> Émile Meyerson, *Identity and Reality* (London: Allen & Unwin, 1930); see App. 1: "Leibniz, Newton, and Action at a Distance," pp. 447–456. According to Meyerson, Newton's objection was not to action at a distance *per se* but, rather, to the belief that this could be a concomitant power of bodies—requiring no further explanation—rather than a power that had to be invested into body by God. My own attempts to show what Newton was really objecting to in this passage have either fallen on deaf ears or have been rejected (though so far only in personal communications to me and not—as far as I know—in print). See John Henry, "'Pray Do Not Ascribe That Notion to Me': God and Newton's Gravity," in *The Books of Nature and Scripture: Recent Essays on Natural Philosophy, Theology, and Biblical Criticism in the Netherlands of Spinoza's Time and the British Isles of Newton's Time*, ed. James E. Force and Richard H. Popkin (Dordrecht: Kluwer, 1994), pp. 123–147.

<sup>4</sup> John Tyndall, *Faraday as a Discoverer* (London, 1868), p. 90; and Michael Faraday, "Speculation Touching Electric Conduction and the Nature of Matter," *Philosophical Magazine*, 1844, 24:136–144. It is worth noting that matter that is held to be "mutually penetrable" comes closer to earlier concepts of spirit than to traditional views of matter. On the background of work by Boscovich and Priestley see, e.g., Peter M. Harman, *Energy, Force, and Matter: The Conceptual Development of Nineteenth-Century Physics* (Cambridge: Cambridge Univ. Press, 1982), pp. 76–79.

alley, or into a counterfactual tradition of thought or practice (be it as eccentric as we can imagine), would halt, or significantly divert, the inexorable progress of science. According to this point of view, there can be no single pivotal moment upon which all else hinges. Furthermore, upholders of these claims would insist, although contingency might make a difference to the details of the history of science, so that we might be celebrating a Leibniz where now we celebrate a Newton, for example, it would make no significant difference to the bigger picture and to the ultimate achievement of modern science—its achievement so far and its potential to go on to achieve yet more.

The obvious way to defend such a positivist view of science is to point to the phenomenon of so-called simultaneous discovery. The history of science can be presented as a history of scientific discoveries, and so removing a particular discoverer or moment of discovery might seem like an obvious way to set off on a counterfactual history—but simultaneous discovery seems to suggest otherwise.<sup>5</sup> The best discussion of this phenomenon remains Thomas S. Kuhn's "Energy Conservation as an Example of Simultaneous Discovery."<sup>6</sup> Kuhn suggested that a full explanation as to why the concept of energy conservation was arrived at independently by several physicists in the mid-nineteenth century would call attention to the community-wide distribution of several factors, including an awareness of conversion processes, a concern with engines, and what he called the prevailing "philosophy of nature" (in this case, as it happened, *Naturphilosophie*). Equivalents of energy conservation in the period of the Scientific Revolution might include the development of the concept of inertia. Although it is possible to insist that the full-blown concept of inertia did not appear before Newton, there can be no denying that Galileo, Pierre Gassendi, and Descartes played a role in opposing the Aristotelian concept that everything that moves must be continuously moved by something else and in suggesting instead that once something was moving perhaps it might simply carry on moving until something else stopped it.<sup>7</sup> Moreover, these are only the big names, the ones who have attracted scholarly attention. It seems likely that, if we took the trouble to look, we could find lesser thinkers who were trying to develop ideas about motion along the same lines.

Similarly, the prevalence of quasi-atomistic theories of matter and body in the Scientific Revolution lead one to suppose that, here again, there was something (or some things) in the air that ensured that Descartes was not the only one to build a mechanical philosophy around them nor Daniel Sennert and Robert Boyle the only ones to build a chemical theory around them.<sup>8</sup> The "mathematization of the world picture," long recognized as a *sine qua*

<sup>5</sup> The best example is Daniel J. Boorstin, *The Discoverers: A History of Man's Search to Know His World and Himself* (New York: Random House, 1983). For a sophisticated historiographical investigation as to how discovery accounts have been created by historians of science see Simon Schaffer, "Making Up Discovery," in *Dimensions of Creativity*, ed. Margaret A. Boden (Cambridge, Mass.: MIT Press, 1994), pp. 13–51.

<sup>6</sup> Thomas S. Kuhn, "Energy Conservation as an Example of Simultaneous Discovery," in *Critical Problems in the History of Science*, ed. Marshall Clagett (Madison: Univ. Wisconsin Press, 1959), pp. 321–356; this essay is now most easily accessible in Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: Univ. Chicago Press, 1977), pp. 66–104.

<sup>7</sup> Alan Gabbey, "Force and Inertia in the Seventeenth Century: Descartes and Newton," in *Descartes: Philosophy, Mathematics, and Physics*, ed. Stephen Gaukroger (Hassocks, Sussex: Harvester, 1980), pp. 230–320.

<sup>8</sup> On the varieties of sources feeding into the atomist revival see, e.g., Andrew G. van Melsen, *From Atomos to Atom: The History of the Concept Atom* (New York: Harper, 1960); and Christoph Lüthy, John Murdoch, and William R. Newman, eds., *Late Medieval and Early Modern Corpuscular Matter Theories* (Leiden: Brill, 2001). For Sennert and Boyle—and others—as chemical atomists see Newman, *Atoms and Alchemy: Chymistry and the Experimental Origins of the Scientific Revolution* (Chicago: Univ. Chicago Press, 2006).

*non* for the Scientific Revolution, used to be told in terms of a pantheon of god-like heroes of science—Copernicus, Galileo, Kepler, Descartes, Newton—but now it is seen in terms of the changing social status of mathematical practitioners and concomitant changes in attitudes about the relevance and value of mathematics, in everyday life but also in the higher echelons of thought.<sup>9</sup> No longer the preserve of a few heroic contributors, the mathematical approach to understanding is now seen as a pervasive worldview in the period. From the perspective of the counterfactual historian, no mathematical genius was indispensable. We knew, thanks to Leibniz, that we would have had infinitesimal calculus even if there had never been a Newton; but it now appears that we could do without Leibniz too—sooner or later, somebody was going to develop the technique.

To accept the claim that science has a built-in guidance system that will lead it ever onward to the truth, and to deny that the development of science is dependent on mere contingencies, seems to reflect a belief that science, like the truth, will out. The scientific method is such that, whatever the weaknesses of human endeavor, scientific truths will steadily emerge and will come to be recognized and established as a matter of inevitability. Almost no matter what contingencies are “thrown” at science, it is self-correcting—nothing can throw it off track. To change science would require nothing less than to change the nature of the world. As long as the world remains the same, there is only one science that explains it, and the history of science is the history of how we gradually discovered that one science and how we are continuing to discover previously unrealized aspects of it.

Most historians of science nowadays would immediately want to distance themselves from this kind of positivistic attitude to science. And yet, as we’ve already seen, the dominant trends in recent history of science have provided a great deal of support for this positivist approach. In professional history of science there has been a move away from “great man” history, and away from heroic discovery accounts, and a move toward establishing the kinds of factors Kuhn discerned as necessary to account for the simultaneous discovery of energy conservation. That is to say, the main impulse now is to understand the background or context of scientific change, from the most general level of the prevailing “philosophy of nature” to the more restricted levels of, say, professional concerns, personal interactions in the work space, or the preoccupations generated by prevailing scientific theorizing.<sup>10</sup> The focus, for the most part, is on those aspects of the history of science that can be said to be socially constructed.<sup>11</sup> But even the social constructivist wants to lay claim, at least to some extent, to the inevitability of the

<sup>9</sup> The phrase was first coined by Alexandre Koyré but was taken up by virtually all succeeding commentators on the Scientific Revolution. It was echoed, e.g., in E. J. Dijksterhuis, *The Mechanization of the World Picture* (Oxford: Oxford Univ. Press, 1961). The new historiography of mathematics in the Scientific Revolution effectively began with Robert S. Westman, “The Astronomer’s Role in the Sixteenth Century: A Preliminary Survey,” *History of Science*, 1980, 18:105–147. For other examples consider Nicholas Jardine, “Epistemology of the Sciences,” in *The Cambridge History of Renaissance Philosophy*, ed. C. B. Schmitt and Quentin Skinner (Cambridge: Cambridge Univ. Press, 1988), pp. 685–711; Mario Biagioli, “The Social Status of Italian Mathematicians, 1450–1600,” *Hist. Sci.*, 1989, 27:41–95; and Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: Univ. Chicago Press, 1995).

<sup>10</sup> Some even take this to the extent of studying science “from below,” looking at the rank and file and their involvement in what in Kuhnian terms would be regarded as “normal science.” For a recent example see Deborah H. Harkness, *The Jewel House: Elizabethan London and the Scientific Revolution* (New Haven, Conn./London: Yale Univ. Press, 2007).

<sup>11</sup> For surveys of these kinds of history of science see Steven Shapin, “History of Science and Its Sociological Reconstructions,” *Hist. Sci.*, 1982, 20:157–211; and Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science* (Cambridge: Cambridge Univ. Press, 1998).

historical development of science. The alternative would be to accept that efforts to uncover the equivalents of Kuhn's three general factors underlying the multiple discovery of energy conservation are simply irrelevant in helping us to understand the development of science. Clearly, the social constructionist historian of science wants to offer an account that is seen as causal; and, given that that account is couched not in terms of a single stroke of genius but in terms of a pervasive set of social concerns, it seems hard to deny the suggestion that the development of science is inevitable if these social concerns are dominant.

Social constructivism and positivism are usually considered to lie at opposite ends of the spectrum of metascientific studies; and yet, paradoxical though it may seem, with regard to the significance of contingency in the history of science they seem to be closely allied. It is one thing to show that Copernicus was a unique genius whose loss from history might have had crippling consequences for the development of the natural sciences; it is quite another to show that attitudes to the relevance of mathematics to natural philosophy were radically changing throughout the Renaissance and giving rise to new kinds of mathematical practitioner and to new claims about what mathematics could tell you about the natural world. The latter scenario makes it entirely reasonable to suppose that if Copernicus had never been born somebody else, sooner or later, would have inaugurated the astronomical revolution. It is the recent emphasis in the historiography of science on the social milieu rather than on the roles of individual thinkers, therefore, that has unwittingly provided support for positivistic approaches to science and its history.

Unique though Isaac Newton was, it seems hard to believe that others would not have moved mathematical physics in roughly the same direction. Robert Hooke had the idea of the universal principle of gravitation before Newton, and though he didn't have the mathematical skills to prove its truth there were surely others in Continental Europe who did.<sup>12</sup> A *Principia mathematica* written by Leibniz certainly could not have been the same as Newton's, and indeed would have differed from it very fundamentally in terms of its "philosophy of nature," and yet it might well have had an essentially similar influence on the subsequent development of mathematical physics. After all, we now know that the real Isaac Newton was very different from the image created by subsequent generations of physicists. The last of the magi became a new Newton to suit succeeding ages: a Newton who did not believe in alchemy and who did not believe in actions at a distance. If Leibniz had to substitute for Newton in a counterfactual history of physics, we could easily imagine that the real Leibniz would simply have been turned into the same kind of embodiment of the Age of Reason that Newton was. This strongly suggests that there was a *Zeitgeist* at work. The scientific *Zeitgeist* asserts itself, no matter what materials it is given to work with. If we ask ourselves where the *Zeitgeist* comes from, and how it got to be that way, we find ourselves at a loss—unless, of course, we are positivists. For the positivist the *Zeitgeist* is shaped by the gradually unfolding truths of science. This is why, the positivist would say, even so towering a genius as Newton cannot assert his own values on the *Zeitgeist*. Eighteenth-century natural philosophers had their own convictions as to what was good and useful in Newton's approach and in his output, convictions justified (presumably) by their assessments of the best scientific achievements of the day.<sup>13</sup>

<sup>12</sup> See the forum "Reconsidering the Hooke–Newton Debate on Gravitation: Recent Results," ed. Niccolò Guiccardini, in *Early Science and Medicine*, 2005, 10:510–543.

<sup>13</sup> But clearly their assessments did not come from noticing that Newton was an alchemist—otherwise they would (surely?) have decided that alchemy was the way to go. The mystery remains as to what shaped



They would not be deflected from these convictions by what for them would have been the inconvenient truth that Newton achieved what he did because he combined the mechanical philosophy with an empirically justified occultism. It was simpler for them simply to cast Newton in the mold they wanted him to fit.<sup>14</sup>

It would seem, then, that there is an unholy alliance between social constructivism and positivism in the history of science that militates against the significance of contingency in the development of the sciences. For both parties to the alliance, contingent changes would amount only to a change in specific details; they would make no real difference to the development of science. Commentators on science seem to favor the view that modern science will turn out the same, come what may.

But, of course, our discussion so far disguises a very important distinction between positivists and social constructivists. Whereas the positivist presumably rejects the significance of contingencies in principle, and would wish to suggest that no counterfactuals would make any significant difference to the development of science, the social constructivist historian objects only in practice. Most practicing historians would agree with Kuhn about the way to understand the development of energy conservation or any other aspect of science, and to that extent they would tend to deny the relevance of small contingent changes. They would, however, be willing to entertain the notion that science and its history could have been radically different if we suppose sweeping changes in our counterfactual history.

Historians of the Scientific Revolution periodically exercise themselves over the issue as to why the Scientific Revolution occurred when and where it did. At least at a superficial glance, it is hard to understand why things did not take off in Hellenistic Greece, or in Imperial China, or in the early centuries of the Islamic ascendancy.<sup>15</sup> It is easy to see that if the major focus of science historians' concerns was not "Western science" but "Chinese science," "Islamic science," or whatever other designation might have been coined in a world with a completely different history, then we would be looking, surely, at a very different kind of science.<sup>16</sup> Perhaps positivists would feel obliged to deny this and to claim that, nevertheless, the science would be substantially the same. But speculations about the why and the when of the Scientific Revolution give rise to another possibility: that modern science may never have emerged at all.

The advanced knowledge of the natural world possessed by the ancient Greeks meant little to the Romans, and for a long time that knowledge went into decline. Similarly, the advanced knowledge of the Chinese and of the Arabs failed to go on to greater things. Just

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Enlightenment attitudes to the sciences. It should perhaps be noted that although Newton kept his alchemy secret, he did not keep secret the fact that he was an alchemist—it was well known to contemporaries.

<sup>14</sup> On Newton as a mechanist occultist see R. S. Westfall, "Newton and Alchemy," in *Occult and Scientific Mentalities in the Renaissance*, ed. Brian Vickers (Cambridge: Cambridge Univ. Press, 1984), pp. 315–335; and John Henry, "Occult Qualities and the Experimental Philosophy: Active Principles in Pre-Newtonian Matter Theory," *Hist. Sci.*, 1986, 24:335–381.

<sup>15</sup> On the possibilities in ancient Greece see Lucio Russo, *The Forgotten Revolution: How Science Was Born in 300 BC and Why It Had to Be Reborn*, trans. Silvio Levy (Berlin: Springer, 2004). On China see G. E. R. Lloyd and Nathan Sivin, *The Way and the Word: Science and Medicine in Early China and Greece* (New Haven, Conn.: Yale Univ. Press, 2002). On China and Islam see Toby E. Huff, *The Rise of Early Modern Science: Islam, China, and the West* (Cambridge: Cambridge Univ. Press, 1993); and H. Floris Cohen, *The Scientific Revolution: A Historiographical Inquiry* (Chicago: Univ. Chicago Press, 1994).

<sup>16</sup> Consider, e.g., the differences between the way Asians and Westerners think, as outlined in Richard E. Nisbett, *The Geography of Thought: How Asians and Westerners Think Differently . . . and Why* (New York: Free Press, 2003). See also the final section of John Henry, "National Styles in Science: A Factor in the Scientific Revolution?" in *Geography and Revolution*, ed. David N. Livingstone and Charles W. J. Withers (Chicago: Univ. Chicago Press, 2005), pp. 43–74.

why the incipient science of the Renaissance became so successful that it was able to sustain itself and eventually establish itself as a characterizing feature of Western civilization is a mystery that has never been satisfactorily resolved. It is almost certain that the explanation depends on the concatenation of a number of factors. It is perfectly possible, therefore, that the coincidence of these factors might never have occurred, and Renaissance science might have petered out just as Chinese and Islamic science had before it. In that case, we would be living in a very different world. It might be worth remarking, however, that if we do manage to come up with a historical explanation of all the factors involved in accounting for the burgeoning of science, beginning in the Renaissance, then there would be a strong tendency to suppose that, given all these factors, the rise of science was inevitable.

So, provided we make sure that our counterfactual examples are wide ranging and entail a completely different social milieu of their own, including what Kuhn would have called a completely different “philosophy of nature,” we can separate ourselves from positivists and continue to imagine counterfactual histories in which science would have turned out very differently. We cannot, however, hold that small changes in the actual history of science would have made a difference without simultaneously invalidating the historiography of science of the past half century or so. To do so would be to suggest that the wider social milieu of the sciences is not really a significant factor in understanding their development.

Speculations that come close to the historiographically correct kind of wide-ranging counterfactual history of the Scientific Revolution have already been put forward by feminist historians of science. The starting point for these suggestions seems to have been the pioneering study by Evelyn Fox Keller in 1978 on the gendered nature of Western science. After lamenting the effective exclusion of women from the sciences, Keller suggested that “were more women to engage in science, a different science might emerge.” She subsequently went on to develop the idea that the masculinization of science began in the Scientific Revolution and could be demonstrated by a feminist reading of the works of Francis Bacon. Keller’s speculations were soon underpinned by the detailed historical studies of Carolyn Merchant, in her immensely influential *The Death of Nature*. Although neither Keller nor Merchant specifically discusses counterfactual histories, there are strong hints in their work that science might have been very different indeed if women had played a greater role in its formation and development. Certainly, Merchant presents a picture of a different kind of premodern science, in which women did take part, and which was more holistic and organic than the post-Baconian and post-Cartesian science that effectively suppressed this more feminine science. Given the picture she paints, it is easy to imagine that science might have been very different, if the social milieu of which it was a part had conformed more closely to the social ideals of post-1960s feminists.<sup>17</sup>

In spite of efforts by a number of leading feminist philosophers, it is by no means clear

<sup>17</sup> Evelyn Fox Keller, “Gender and Science,” *Psychoanalysis and Contemporary Thought*, 1978, 1:409–433; rpt. in Keller, *Reflections on Gender and Science* (New Haven, Conn.: Yale Univ. Press, 1985), pp. 75–94, on p. 76. For a feminist reading of the works of Bacon see, e.g., Keller, “Baconian Science: A Hermaphroditic Birth,” *Philosophical Forum*, 1980, 11:299–307; revised version rpt. as “Baconian Science: The Arts of Mastery and Obedience,” in Keller, *Reflections on Gender and Science*, pp. 33–42. For Merchant’s presentation see Carolyn Merchant, *The Death of Nature: Women, Ecology, and the Scientific Revolution* (San Francisco: Harper & Row, 1980), esp. Chs. 1–6. Although I admire this book very much, I have to say that I do not subscribe to its claims about the nature of premodern science.



what this alternative, and supposedly very different, feminist science would look like.<sup>18</sup> One thing is clear, however; and that is that feminist writers have not sought to establish their claims by producing counterfactual histories of what might have been. Clearly, counterfactual histories are just too speculative, too obviously based on flights of fancy, to establish the truth of whatever claims might inspire them. Feminist beliefs about a radically different feminist science might have been inspired by Merchant's hints in *The Death of Nature*, but they could never be confirmed by imaginary histories. Furthermore, it is obvious that this point can be generalized. The point of the history of science should be to help us to understand the nature of science itself: its methods, its "logic of discovery," how consensus is reached, how controversies are settled, whether (or to what extent) its institutional forms directly affect its doctrines and practices, the relationship between its theories and its practices, and so forth. It is hard to see how counterfactual histories could help with any of these things, especially counterfactual histories that are so far removed from our living histories (as the radically contingentist counterfactuals have to be, according to the Kuhnian ethos of explanation discussed here) that we can hardly imagine what the alternative scenarios would be like.

<sup>18</sup> See, e.g., Sandra Harding, *The Science Question in Feminism* (Milton Keynes: Open Univ. Press, 1986); Hilary Rose, "Beyond Masculinist Realities: A Feminist Epistemology for the Sciences," in *Feminist Approaches to Science*, ed. Ruth Bleier (New York: Pergamon, 1986), pp. 57–76; Elizabeth Fee, "Critiques of Modern Science: The Relationship of Feminism to Other Radical Epistemologies," *ibid.*, pp. 42–56; and Helen Longino, "Subjects, Power, and Knowledge: Description and Prescription in Feminist Philosophies of Science," in *Feminism and Science*, ed. Evelyn Fox Keller and Longino (Oxford: Oxford Univ. Press, 1996), pp. 264–279.